

Dear Arthur,

I am sorry not to have
replied more quickly to your letter
dated 7th October, but the term has
been rather hectic, as I have been
lecturing both at Oxford and in London
and dashing back and forth,
between the two places!

Now that term has ended I have
settled back to read your interesting
paper on Correlations and Physical Work.
~~This throws quite a lot of light on the
points you were making about the~~

But just let me explain my position
about your 1977 paper that the Correlation
together with Conservation and Foot, leads
to inconsistency.

The assumption of Locality comes in, as I
think, because you do not allow the possibility

that the value of $I \otimes B$ in the state $U(\phi \otimes \xi)$ depends on whether you are measuring A or $f(A)$ on the first part. If you did not measure locally you would have to wait.

$$(1) [A \otimes I]_B^{U(\phi \otimes \xi)} = X_m \Rightarrow [I \otimes B]_A^{U(\phi \otimes \xi)}$$

and also

$$(2) [I \otimes B]_{f(A)}^{U(\phi \otimes \xi)} = Y_m \Rightarrow [f(A) \otimes I]_B^{U(\phi \otimes \xi)}$$

where you see notation $[I \otimes B]_A^{U(\phi \otimes \xi)}$ to indicate the value of $I \otimes B$ in the state $U(\phi \otimes \xi)$ when the apparatus is set to measure A on the first particle, etc.

But from (1) and (2) I cannot deduce

$$[f(A) \otimes I]_B^{U(\phi \otimes \xi)} = f([A \otimes I]_B^{U(\phi \otimes \xi)})$$

Simply because.

$$\{I \otimes B\}_A^{U(\Phi \otimes \mathbb{I})} = \gamma_m \quad \nabla$$

$$\{I \otimes B\}_{f(A)}^{U(\Phi \otimes \mathbb{I})} = \gamma_m.$$

This implication only goes through,
if we don't distinguish $\{I \otimes B\}_A^{U(\Phi \otimes \mathbb{I})}$ from
 $\{I \otimes B\}_{f(A)}^{U(\Phi \otimes \mathbb{I})}$ as in your published version
and this is what I meant by saying you
assumed locality.

I would then want to agree that
your 1977 proof of inconsistency is
really a proof of nonlocality, and

we decide to hang on to the plausibility
of Causation (until after all is a particular
case of the extended value rule for quantum
compossible observables, which you allowed in
your 1974 Synthese paper -)

So let me know what you think about this.

Now let me make a few comments on your new paper:

p.19. In your discussion of resolvable indeterminism, do you mean that the probabilities for each λ so captured by (CH) is that $p(ST, \lambda)$ is itself expressible in the ~~form~~ ~~form~~ form

$$p(ST, \lambda) = \int_0^1 S(x) T(x, \lambda) dx$$

In other words, in terms of a space of derived pairs $\langle x, \lambda \rangle$ with a product measure defined as it derived from the uniform measure on x and the P -measure λ , we are writing

$$\begin{aligned} \phi(S, T) &= \int_{\Lambda} p(ST, \lambda) e(\lambda) d\lambda \\ &= \int_0^1 \int_{\Lambda} S(x, \lambda) \cdot T(x, \lambda) e(\lambda) dx d\lambda \end{aligned}$$

So factorization has been entered at the $\langle x, \lambda \rangle$ level of description.

Now this is what I understood Shimony
 to be claiming, that at a suitable
 level of description fortuitous fields
 and its policies for any given level
 of description is an indicator that the
 level is not refined enough. Your
 discussion of especially indeterminate
 seems actually to bear out Shimony's
 claim, although I take it that your
 regard your discussion as a counter
 to Shimony's support of CH in
 linking locality with fortuitous
 I am genuinely confused and would
 appreciate further clarification.

p. 20 I am not happy with your discussion
 of Nelson's theorem. It is ambiguous what
 you mean by the remark 'each degree
 \bar{A}_5 is more or less compared to the random
 variable A_5 ! If this means A_5 is close
 so that $\text{Prob}_{\delta H} [\Delta]_{A_5}^2 = \text{Prob}_{\text{i.v.}} (A_5^{-1}(\Delta))$

for all B and $x \in B$, where I now use the notation $\{A_i\}_{i=1}^n$ for the proposition that A_i is a value belonging to the set A and so on.

i.e. if you assume that A_i gives the right probability distribution for A_i according to the statistical algorithm of QM, then it follows that

$$\langle \tilde{A}_i \rangle_{qm} = \langle A_i \rangle_{n.v.}$$

$$\begin{aligned} \text{and } \langle \tilde{S} \rangle_{qm} &= d_1 \langle \tilde{A}_1 \rangle_{qm} + d_2 \langle \tilde{A}_2 \rangle_{qm} + \dots \\ &= d_1 \langle A_1 \rangle_{n.v.} + d_2 \langle A_2 \rangle_{n.v.} + \dots \\ &= \langle d_1 A_1 + d_2 A_2 + \dots \rangle_{n.v.} \\ &= \langle S \rangle_{n.v.} \end{aligned}$$

But this makes your statement of Nelson's theorem trivially false. What Nelson did show was that if A corresponds to \tilde{A} in the sense of the correspondence then there always exists a choice of the coefficients d_i such that the probability distributions for S and \tilde{S} do not agree, (although the expectation values will agree,

What Bell's argument shows I would have
thought, is that all random variables X
correspond to $\hat{A} \hat{B}$, $\hat{A} \hat{B}'$, etc. cannot be
just $A(\lambda) B(\lambda)$, $A(\lambda) B'(\lambda)$, etc. taken
 ~~$A(\lambda)$ as the random variable corresponding to~~
 ~~\hat{A} and $B(\lambda)$ to \hat{B} , etc. (using~~
~~throughout my sense of the word 'conspiracy'~~

~~I fail to see the connection here with~~
~~Nelson's theorem. even if the correspondence~~
~~is restricted to getting only the expectation~~
~~values right. I fail to see the~~
~~connection here with Nelson's theorem~~

p.22 ft.

I admire the ingenuity of your synchroniz-
ed prism models. With regard to the
former I feel the term 'conspiracy' might
might be more appropriate, if they could
reproduce QM predictions in all circumstances
and never allow the two 'possessed' detectors

to be proved! With regard to the prem
models I agree that this is possible
but feel that ~~decoherence~~ 'decoherence',
and perhaps we must feel up to
'decoherence' in quantum mechanics

It was a great pleasure to meet
you again last summer. May I
wish you and your family a happy
1981.

Yours ever.

Richard